As regards the figures that accompany the work itself, it is a matter for regret that they cannot be commended. Few objects are more difficult to draw or require more skill in their delineation than do the shells of mollusca, and the amateur is rarely able to do them justice. The want of finish in the present instance is all the more noticeable from the contrast they afford to the rest of the "get up" of the work, which is admirable.

These shortcomings are not thus dwelt on in any captious spirit, but are pointed out in the friendly hope that a future edition of the work may shortly be forthcoming, in which the defects of the present one, compiled under great difficulties and at much disadvantage, may be made good and a really complete catalogue result.

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OUR BOOK SHELF.

The Framework of Chemistry. Part I. By W. M. Williams, M.A. (London: George Bell and Sons, 1892.)

THIS is the first part of a book which has been specially written as a supplement to the oral lessons and experimental demonstrations given by a teacher. It is intended to contain nothing but what is absolutely necessary to give definite and precise impressions regarding the salient points of the lessons, all details relating to laboratory manipulation being omitted. The more important introductory facts, divested of theoretical considerations, are first discussed, then come "atoms and molecules," treated in an elementary fashion and leading the way to the explanation of the use of symbols and formulæ.

How the system adopted by the author will work out can only be ascertained when the other parts are to hand. So far as the information in the present volume goes, it is to a great extent useful and clearly stated.

Objection may be taken to the classification of solutions as mechanical and chemical, for, were it for no other reason, it is still a disputed point whether any solution may be considered a mixture.

The concise style of the book lends itself to incomplete statements. For instance, to say that one of the oxides of carbon "contains exactly twice as much oxygen as the other," is hardly accurate; a constant quantity of carbon is essential to the accurate conception of the facts. The most serious blunder made by the author lies in the confusion of force and energy. This is manifest in statements involving the conversion of "chemical force" into an "equivalent amount of heat" or of "electrical force," and culminates in the assertion that "Force, like matter, cannot be destroyed."

The Beauties of Nature, and the Wonders of the World we Live In. By the Right Hon. Sir John Lubbock, Bart. M.P., F.R.S. (London: Macmillan and Co., 1892.)

SO many writers of the present day adopt a pessimistic tone that a pleasant impression is always produced by Sir John Lubbock's genial and imperturbable optimism. In the present volume he undertakes to show how many sources of interest men might find in the world around them, if they would only take the trouble to train themselves to appreciate the scientific significance of ordinary facts. He begins with a study of animal life, and has much that is fresh and suggestive to say about various aspects of the subject. Then there are chapters on plant life, woods and fields, mountains, water, rivers and lakes, the sea, and the starry heavens. The volume is written in the clear, frank style with which all readers of Sir John Lubbock's books are familiar, and it ought to do much to foster among the class to which he appeals habits of careful and exact observation. His readers have the satisfaction of knowing that of the many things they may learn from him none will afterwards have to be unlearned.

Algebra for Beginners. By H. S. Hall and S. R. Knight. (London: Macmillan & Co. 1892.)

THIS work is intended as an "easy introduction" to the author's "Elementary Algebra for Schools," and, besides being treated on lines similar to those of the last-mentioned book, is published in a cheaper form. The idea throughout seems to have been to present the beginner with the practical side of the subject, and with this intention the examples are made as interesting as such examples can be. The usual sequence has not here been strictly adhered to; but a beginner will find that he will still be able to reach the "as far as quadratic equations" limit. It is needless to say that the explanations are stated in clear and simple language, while the examples are all new. That this book will be widely used is undoubted, for it will form an excellent forerunner to the more advanced one referred to above.

Introduction to Physiological Psychology. By Dr. Theodor Ziehen. Translated by C. C. van Liew and Dr. Otto Beyer. (London: Swan Sonnenschein and Co. 1892.)

IN reviewing the book of which this is a translation (NATURE, vol. xliv. p. 145), we pointed out that such a book was badly wanted in English. We are glad, therefore, to welcome a translation of Dr. Ziehen's work, which will serve well as an introduction to the new science of physiological psychology.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The *Volucellæ* as Examples of Aggressive Mimicry.

AN interesting point in the *Volucellæ* as examples of aggressive mimicry is the fact that they were first used to support the teleological theories of an earlier day, and were subsequently claimed by natural selection. Thus Messrs. Kirby and Spence speak of them (Second Edition, 1817, vol. ii., p. 223) as affording "a beautiful instance of the wisdom of Providence in adapting means to their end;" and after describing the resemblance of the flies to the bees, they continue, "Thus has the Author of nature provided that they may enter these nests and deposit their eggs undiscovered. Did these intruders venture themselves amongst the humble-bees in a less kindred form, their lives would probably pay the forfeit of their presumption." In this theory of Providence it is hard to see where the bees come in. In 1867, A. R. Wallace published an article on "Mimicry and other Protective Resemblances among Animals," which was in 1875 republished in his "Essays on Natural Selection." In this essay (p. 75 of the volume) he spoke of this interpretation as the only case in which an example of mimicry had been "thought to be useful, and to have been designed as a means to a definite and intelligible purpose." He accepts it as a product of natural selection, and since that time it has been constantly used as a well-known example of this principle, so well known, indeed, that the history of it became unnecessary in any publication where space was an object.

I neither originated the principle of aggressive mimicry nor the *Volucellæ* as examples of it, although I accepted, and still accept, both. Under these circumstances I must, in justice to Kirby and Spence and A. R. Wallace, repudiate the discovery of a significance I should have been proud to have made, but which was made, as a matter of fact, about half a century before I was born. It is only fair to these writers to say this, for Mr. Bateson, although mentioning Kirby and Spence, seems throughout to give me the sole, or, at any rate, the chief, responsibility for both hypothesis and examples.

In writing my book I made great use of a very interesting series of specimens in the Museum of the Royal College of Surgeons, lately brought together by Prof. Stewart. The aggressive mimicry of the *Volucellæ* was illustrated in one of these cases, and I briefly described the contents of the case in the passage Mr. Bateson quotes. I was glad to give a few more details than those supplied by Mr. Wallace, and at the same time to mention examples which could be actually seen by readers; for I referred to the collection more than once. I was, however, anxious to obtain confirmation from one who had studied the Hymenoptera and their parasites much more minutely than I had, so I referred the proofs to Mr. R. C. L. Perkins, a most observant naturalist, specially interested in these insects. He made some valuable suggestions, but did not modify the account of the case in the Royal College of Surgeons. I think I may claim, therefore, that I took all reasonable precautions to avoid error in a part of the subject which had not then come under my own personal observation. Prof. Lloyd Morgan, in his interesting "Animal Life and Intelligence," has also mentioned this example, and figures the *Volucella* and *Bombus muscorum*. He tells me that his figures were copied from a case in the Natural History Museum, so that my selection appears to be supported by the two great biological museums of London.

Within a few weeks of the appearance of my book, I had found out the omission of the other banded humble-bees also mimicked by the *mystacea* variety of *Volucella bombylans*, and I showed one of these (I think *B. hortorum*) at a lecture given to the British Association at Leeds in 1890. I had intended, and intend, to repair the omission in any reprint that may be required.

There is, however, nothing inaccurate in the statement that *B. muscorum* is mimicked. We require something more than dogmatic assertions and question-begging metaphor of tabby-cat and fox to establish this as an error of the two museums and the two volumes which have followed them in this respect. Mr. Bateson appears to have been studying the literature of *Volucellæ* rather carefully; if he now extends his investigations to the perfect insects themselves, and compares the individuals in a series of moderate length, he will find that the *mystacea* variety differs much in the demarcation of its rings or zones, and also that the appearance of each individual varies with the direction from which it is observed. The less sharply-marked appearances resemble *B. muscorum*, the likeness being increased by a slight indication of zoning to be seen in the latter.

On July 7 of the present year I captured, in a wood near Newbury, a pair of the variety *mystacea* in copula. The male, the larger insect, was unusually indistinctly zoned. I have submitted the specimens to Mr. Verrall, who kindly tells me that the large male is certainly the variety *mystacea*, and he evidently thinks there is nothing remarkable about it. On the other hand, the female, which was unusually small, is more interesting, being somewhat an intermediate variety. As, Mr. Verrall informs me, Rondani has made about a score of intermediate species, this little capture of mine may turn out to be of interest, and it is comforting in a controversy of this kind to be able to add one fresh observation which may be of some use, if only in the way of confirmation.

Now as to the statement, in which no ambiguity was intended, that the two varieties lay in the nests of the bees they respectively mimic. This was, as Mr. Bateson says, a very general impression, the impression of naturalists who knew these insects far better than I did, an impression which had already been expressed in the case at the College of Surgeons. If I was mistaken in adopting it, was it not well that I made the mistake, if by its means the general impression should be corrected, having in my book assumed a tangible shape? What man who cares for the advance of science more than for his own advancement would regret to have made a mistake under such circumstances?

But I am not yet satisfied that the impression is not substantially correct. I do not regard the dimorphism of *V. bombylans* as the unique phenomenon it appears to be in the opinion of Mr. Bateson. I fail to see any essential biological difference between it and the dimorphism of many Lepidopterous larvæ—a dimorphism which extends into the pupal stage of most species of the genus *Ephyra*—or between it and the distinct types into which certain butterflies of the genus *Kallima* can be divided according to the colouring of the under sides of the wings, or certain moths of the genus *Triphæna* according to that of the upper sides of the upper wings. But we know that in those cases which have been tested, while the majority of the offspring resemble the variety to which the parents belonged, a certain proportion follow the other variety, and when the parents belong to different varieties the offspring are more equally divided. It is therefore only to be expected, so far as our present knowledge goes, that both varieties should emerge from the same nest. The important thing to be ascertained, from the point of view of the theory of aggressive mimicry, is not the colour of the offspring which emerge, although this is of high interest on other accounts, *but the colour of the parents which enter*. It might be supposed that Mr. Bateson would have understood this, but it is perhaps too much to expect from a critic who is so aggressively uninterested.

It would be interesting to know the grounds upon which Mr. Bateson considers the dimorphism of *V. bombylans* to be almost unique. At present he contents himself with assertions. If we were ever to return to the *régime* of authority and dogmatism in place of reason and experiment, Mr. Bateson's scientific position would be indeed assured.

Years ago I was satisfied that the evidence for the statement in my book was insufficient, and this, too, I had intended to modify when the opportunity occurred. In lecturing I have often alluded to the investigation as an interesting one, and only a fortnight ago suggested it to the members of the Natural History Society at Marlborough College. Two years ago I endeavoured to breed *Volucellæ* in the manner described by Mr. Bateson, I am sorry to say without success. I may therefore claim that the statement quoted by Mr. Bateson had produced no paralysis of effort on my part either as regards my own work or that which I have been able to suggest to others.

I may add that the upshot of this inquiry—even if it lead to the conclusion that both varieties lay indiscriminately in the nests of all the species they resemble—would not, in my opinion, remove the *Volucellæ* from their place as examples of aggressive mimicry, but the working of the principle would be more complex. I do not, however, propose to render myself liable to further sneers about "ingenuity" by discussing it on the present occasion.

Mr. Bateson's letter appropriately ends by putting into my mouth a defence I should never have advanced—a defence which was obviously inserted in order to impute discredit—and then proceeding to the easy task of demolishing it. Let me therefore say that a mistake is to me a mistake, whether in a volume intended for the public or a paper presented to a scientific society. Indeed, I regret the former more than the latter. Unfortunately, too, mistakes are more liable to occur in the volume, because the ground is wider, and passes in some directions into less familiar regions. But I can honestly say that I have always done my best to avoid mistakes, and that I correct them as the opportunity arises, in fresh papers or in reprints of volumes. And I derive much comfort from Mr. Phelps' dictum, which I am sure appeals to every one who works, that "people who never make mistakes never make anything." EDWARD B. POULTON.

Oxford, October 24.

P.S.—I wish to take this opportunity of correcting certain mistakes in my book ("Colours of Animals," Internat. Sci. Ser.), as it may be some time before the book can be reprinted, owing to the number of copies struck off.

Pages 49, 50.—Dr. Hurst informs me that my abstract of Weismann's work on seasonal dimorphism is wrong. This will be carefully reconsidered in any reprint.

Page 73.—I wish to withdraw the account of *Phrynocephalus*. Although the structures alluded to are probably alluring, there is not sufficient evidence as to the manner in which they are used.

Page 85.—Professor Howes calls my attention to the description of the nerve-terminations in pigment-cells.

Page 94, *et seq.*—Sir J. Ross should be Captain James Ross.

Page 105.—I ought to have added that Mr. Sharpe's conclusions are not accepted by Professor Newton.

Pages 142-146.—Mr. Bateson has shown that the white cocoons of *Saturnia* and *Eriogaster* are not due to the white backgrounds employed, but to disturbance of the larvæ. It is still probable that the principle holds in *Halias prasinana*.

Page 156.—For the above reason I withdraw the argument about the cocoons of *Rumia*, although I believe that it still holds if *H. prasinana* be substituted.

Chapters x., xi. should be read in connection with the experiments on Warning Colours since made by Mr. Beddard and published in his volume, "Animal Coloration."

Page 161.—The cockroach is not a good example. As Prof. Weldon pointed out to me, there is no evidence that its unpleasant smell renders it unfit for food. The hive-bee would be a better instance.

Page 193, line 7 from bottom.—*Fibrous* should be *fulvous*.

Page 203, line 6 from bottom.—For *suited for* read *bearing*.

Page 208, line 13 from top.—*Divert* should be *direct*.

Page 224.—I have since heard from Mr. Skertchly that he did not intend the argument which I quote at the bottom of the page to be taken seriously.

Page 236.—*Diadema bolina* should be *D. misippus*, and it and the *Danais* it mimics occur in *three* varieties, not in *two*. I owe this to Col. Swinhoe; the error was copied from Trimen.

E. B. P.

The Geology of the Asiatic Loess.

In the spring and early summer of this year I had the opportunity, in company with Mr. S. B. J. Skertchly, of examining closely the loess deposits of Shantung, stretching from Chefoo to Tsinan, the provincial capital.

The investigation convinced us both that the original loess of China must be regarded as a marine deposit. Subsequent to the time of Mr. Skertchly's leaving the province, on June 17, I was able to supplement these conclusions by the discovery of a band of limestone rocks bored by pholades and crustaceans up to a height of about 1100 feet, above which line no indications of late marine action were visible. The rocks in the locality near Tsinan-fu are carboniferous limestones interbedded with dioritic porphyries, and are still horizontal and unbroken for some thousands of square miles, having received their present contour in pre-loess ages. The dip for hundreds of square miles in this locality seldom exceeds from 2° to 8°. These facts we hope to make the subject of a joint memoir.

The loess of China has, however, been traced almost continuously beyond the limits of the eighteen provinces to the foot of the Pamîrs. West of the Pamîrs loess occurs in the valley of the upper Oxus, probably in the Kizil Kum, and up to the Caspian, and its marine origin requires us to believe in the submergence within late geologic time of the greater part of Central Asia. Most geologists recoil at such a suggestion, and I am in a small minority in accepting the view that the present distribution of ocean and continent is of very recent date. I may, however, in condonation of heterodox views, refer to the position of the argument with regard to the alleged shifting of the terrestrial axis of rotation, which has within the last few years entered on a new phase. When some years ago I presented these views to the Council of the Geological Society of London they were scouted as utterly untenable. Since that time, while English astronomers have held the view that practically the axis of rotation has undergone, within the limits of observation, no change, American astronomers have come to the conclusion that a secular movement is actually in progress. My own geological observations in Europe, North America, and Asia have led me to infer that the North Pole has within recent geological time shifted, and that a shift is in all probability in progress at the present time along a line following approximately the direction of the 70th meridian of west longitude. This shift is not to be taken to involve a change in the direction in celestial space, but is rather a rolling of the earth over its axis, the latter remaining practically stationary.

Dynamical causes sufficient to account for the change of position of the terrestrial poles, and in consequence of the parallels of latitude, seem to me to be at work. Prof. G. Darwin has calculated the probable change in the position of the pole due to an elevation of the bed of the Pacific Ocean, but no one has touched the converse effect of the change of the pole on the relative levels of the oceans and continents. In addition to the cause suggested in the possible elevation of large tracts of continental land, there are other influences at work tending in the same direction. The different distribution of the large masses of ice around the poles, which probably varies within somewhat large limits, and the slow disturbance of equilibrium resulting from the growth of deltas and deep sea deposits, have frequently been adduced. More important still is perhaps the differential influence of tidal friction in retarding the rotation, the effect of which must be sensibly unequal in the two hemispheres north and south of the equator; another cause may be looked for in the action of aerial currents, the effect of which in the northern hemisphere as containing greater masses of elevated land must be greater.

Another potential cause of shifting has never, that I am aware of, been formulated. Although at present of comparatively small influence, it must at various geological periods have been of great importance. It leads on to dynamic considerations of tidal energy beyond the compass of a letter to explain. The relative part played by the sun and moon, as deduced from gravitational formulæ, does not quite agree with the observed phenomena of our daily tides. It is believed by many that the ordinary lunar tide, affecting mainly the oceanic envelope, is complicated by the presence of a terrene tide largely influenced by the sun, and that the earth does to an appreciable extent yield twice in the twenty-four hours to the deforming force of solar gravitation. So long as this oscillation takes place at regularly recurrent intervals no energy is wasted. Should, however, a sudden snap occur, breaking the rhythm of the oscillation, some energy is evidently spent, and this can only be made up from the *vis viva* of rotation. Such snaps do occur occasionally; the regular oscillation is momentarily suspended, and the waters of the ocean rush in to restore the equilibrium. This is the well-known "tidal" wave that so frequently occurs in connection with earthquakes.

Such a snap on the equatorial line would simply retard the rotational period generally. North or south of this line, as the moments of rotation would be instantly unequal, the sphere would roll over its axis of rotation, and a shift in the position of the poles occur. The earth is not a perfectly rigid mass. Were it as rigid as steel, the interior within a depth of 200 miles would yet, under the pressure of gravitation, behave as a liquid; a shift in the pole would then be met either by a corresponding shift in the equatorial protuberance, or a change in the ocean level; or, more probably, by a compound action of both. In the latter case, to fulfil the conditions of equilibrium, the ocean surface in the neighbourhood of the new equator would rise, and if the shift were sufficiently great, would overflow the lowlands. If the equator, in the longitude of Central Asia, had at any former time passed north of its present position, and the rock masses of the Continent had not been elevated, a mid-Asian sea must have resulted. The undisturbed position of the carboniferous rocks, and the plain evidence that the surface sculpturing of the rocks was of pre-loess age, show that the process was unaccompanied by violent movements.

The theory of the shift of the earth over its momentary axis accounts better than any other for the geological condition of polar lands, and I venture to state it again in brief, as on this occasion the initiative has come from the astronomers, not the geologists.

Thos. W. Kingsmill.

Shanghai, China, August 20.

Note on Mr. Kingsmill's paper.

I think it will be difficult for Mr. Kingsmill to adduce evidence of geological changes large enough to produce any considerable shifting of the position of the principal axes of the earth, and accordingly I should feel sceptical as to a theory which postulates that such change has been sufficient to explain considerable changes of climate.

With respect to a later part of the paper, I am entirely at variance with his views. As far as I know "the relative part played by the sun and moon" in producing oceanic tides is in exact accordance with gravitational formulæ.

The existence of a terrene tide is a matter of speculation, but, as the earth cannot be perfectly rigid, it must exist to some extent. The amplitude of the lunar terrene tide must certainly bear to that of the solar the same ratio that holds in the case of oceanic tides, and there is no reason, that I know of, for attributing a greater efficiency to solar action in the case of the deformation of the solid portion of the earth.

I am quite unable to follow the argument by which the so-called "tidal" wave produced by earthquake shock is supposed to produce a retardation of the earth's rotation.

October 21. G. H. Darwin.